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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Nitrous oxide net exchange in a beech dominated mixed forest in Switzerland measured with a quantum cascade laser spectrometer" *by* W. Eugster et al.

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I read the paper with great interest since I am starting to work with the same QCLAS, but for an application on different trace gases. Particularly I was interested in the authors' assessment of the instruments' capacity for continuous eddy covariance flux measurement given a relatively high level of instrumental noise (see Figure 2b).

In my view their thorough spectral analysis and their approach to use the significance of correlation coefficients are attractive ways to assess the feasibility of eddy covariance measurements and are well done. For me the manuscript provides valuable insights into instrument performance and applicability that can be used for my own application.

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Additionally, the discussion on N2O emission, its timing and potential non-soil sources are interesting.

There are a few minor aspects that remained unclear to me and that the authors may want to address:

1.) Calibration scheme: The details of the procedure were not clear to me. I understood they used a two-point calibration (N2, 99.999% and pressurized air) every 30 min. How long was the measurement? Also 20 sec? To determine precision I guess they used a different gas cylinder of dry compressed air measured over 20 sec every 30 min (so: 2 x calibration and 1 x quality control? Each 20 sec?). I would be curious to see if there was any systematic drift (e.g. with temperature) in the measurements of this quality control gas cylinder or of the calibration gases. Also I was wondering if gases for calibration and quality control were sampled at the same flow rate and air pressure or if they sampled at lower flow rates in order to safe calibration gas.

2.) Comparing QCLAS with IRGA LI-7500 (Figure 6): It might be informative to add a little insert to Figure 6 showing a 1:1 plot of measurements with the QCLAS and IRGA.

3.) In results the authors argue that the observed increase in N2O fluxes before precipitation events might be caused by fog or increased atmospheric moisture. Data, however, showing that fog or atmospheric moisture is increasing are missing. Why not adding an extra panel on top of figure 9 showing atmospheric humidity vs. hours since first measured precipitation?

4.) In their conclusion and results the authors argue that “canopy wetting by fog and drizzle must also be a relevant, yet unexplored process leading to N2O emissions from above-ground biomass, probably from senescent leaves”. I am wondering if the authors could strengthen their hypothesis by a more profound literature search on above-ground nitrous oxide emissions in order to discuss potential mechanisms. Several papers seem to discuss this topic.

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Pihlatie M, Ambus P, Rinne J, et al. (2005) Plant-mediated nitrous oxide emissions from beech (Fagus sylvatica) leaves, NEW PHYTOLOGIST 168 (1): 93-98

Rusch H, Rennenberg H (1998), Black alder (Alnus glutinosa (L.) Gaertn.) trees mediate methane and nitrous oxide emission from the soil to the atmosphere, PLANT AND SOIL 201 : 1

Smart DR, Bloom AJ (2001), Wheat leaves emit nitrous oxide during nitrate assimilation, PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES OF THE UNITED STATES OF AMERICA 98 (14): 7875-7878

Interactive comment on Biogeosciences Discuss., 4, 1167, 2007.